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ON THE INHERITANCE OF MAZE BEHAVIOR

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A recent paper on the "Mode of Inheritance of Reaction Time and Degrees of Learning in Mice" by E. M. Vicari (3) purports to demonstrate the inheritance of ability to learn and the dominance or recessiveness of particular types of learning curve. The paper claims attention both because of the importance of these conclusions and because of the dimensions of the experiment itself. Miss Vicari used 900 mice. The four parent strains, referred to as Japanese waltzers, albino, dilute-brown, and abnormal-eyed, had been inbred for a considerable length of time and were apparently of great homogeneity. It was proposed to take a measure of the "reaction time" and the "degree of learning" of these strains (as given in the results with a simple maze) and to look for the presence of strain-characteristics in subsequent crosses. Three of these crosses were made, and Miss Vicari's paper deals with the resulting genetic behavior of the factors involved.

So far as the genetic manipulation is concerned, the experiment calls for no comment, unless, indeed, to point to the considerable advance which it represents over most of the earlier work on the inheritance of behavior. The supposed measures of "reaction time" and "learning," however, are open to serious objection.

Miss Vicari has employed three measures of what she terms "degree of learning." She records "(1) the total number of perfect runs made per mouse for the entire period of training; (2) the highest number of consecutive perfect runs made by each mouse for the entire training; and (3) the number of trials taken by each mouse before making the first perfect run." The data for the four parent strains are given in Table 1. The figures are means for the groups. Miss Vicari gives standard deviations

of the means. These are high, averaging 89% of the mean in (1), 86% of the mean in (2), and 47% in (3).

TABLE 1

Strain	(1) Number of perfect runs	(2) Number of consecutive perfect runs	(3) Number of runs before first perfect run
Japanese waltzing	1.49	0.89	8.80
Albino	1.94	1.43	8.70
Dilute-brown	2.81	1.52	6.90
Abnormal-eyed	1.69	1.15	10.10

In interpreting the table it must be borne in mind that the type of maze was extremely simple. It involved two choices of right or left, and re-tracing was prevented. Purely as a matter of chance we should expect one perfect run in every four and 3.5 perfect runs for the total 14 trials. The observed number falls considerably below this in every case and averages only 1.98 for the four groups. The operation of chance, however, is not considered in Miss Vicari's treatment of her data.

We have to inquire, then, into the nature of the factors operating to deprive the animal of the successful runs which should fall to it by chance. Judging from the late occurrence of the first perfect run, practically all of the successful trials fell in the latter half of the experiment, so that the disturbance, whatever it was, diminished as the experiment progressed. [This does not threaten our contention that the perfect runs occurred by chance, since the number expected in the last seven trials (1.75) is only just exceeded by the observed number for the whole series (1.98). After the first perfect run was made, we should expect to find, in the case of the waltzers, 1.3 perfect runs from the 5.2 remaining. We get only 0.48. For the albinos we expect 1.3 in the 5.2 runs remaining, and get 0.94. For the dilute-browns we expect 1.77 in the 7.1 remaining, and get 1.81. For the abnormal-eyed we expect 0.97 out of 3.9 and get 0.69. The slight positive result for the dilute-brown stock is more than offset by the considerable shortcomings of the other observed figures.]

The only information, then, to be obtained from Miss Vicari's data is that some disturbing influence present in the experimental situation was progressively diminished during the course of the experiment. It would seem utterly unjustified to ascribe any of these conditions to "degrees of learning."

The second method of scoring consisted in measuring the time which elapsed between the animal's release and its entrance into the food box after having passed through the maze. Miss Vicari refers to this as the

"reaction time." Averaging these times for each strain she is able to plot "reaction time curves" which show significant strain differences. The Japanese waltzing mice give records consistently higher than the others. Their curve falls to a minimum at the eighth run and rises slightly thereafter. The albinos, starting just under the waltzers, give a curve which descends gradually throughout the period. The other strains show no significant change in time during fourteen runs.

Although Miss Vicari's use of the phrase "reaction time" is never clear, it seems to be taken quite literally, as if the activity of the mouse in the maze were actually a reaction to a single stimulus, and as if the speed of this response were indicated in the reaction-time. She speaks of "reaction time" as the best criterion of the "maze learning response." In another instance, after finding it necessary to modify the construction of the maze because in its earlier form it encouraged her animals to "curl up in the corner and go to sleep," she says, "The time thus spent could not be counted as a fair reaction to the stimulus." She later defines this stimulus ("or urge used to induce the animal to run the maze") as "hunger." But it is obvious that hunger as a stimulus (if this has any meaning at all) could operate only after the maze had been partially learned. Actually what Miss Vicari has called "reaction time" is the lumped durations of an indefinite number of responses made to an equally indefinite number of stimuli presented by the maze situation. The nature of some of these responses will be noted later.

One thing does come out of her measurements, namely, that there are significant group differences. It would have been surprising if they had not appeared. That there are gross characteristics of behavior which show genetic constancy is, of course, common knowledge to any stock breeder. And that such gross characteristics must have influenced the "reaction times" taken in the present experiment is fully evident. But the mere inheritance of behavioral traits is so patent that there would seem to be no necessity for an experimental demonstration of the fact, unless some means of measurement were devised possessing an advantage over the method of casual inspection upon which our knowledge now rests. There is nothing in Miss Vicari's experimental method which is specifically directed toward the measurement of any given characteristic.

Frequently the investigation of the inheritance of behavior turns to the rubrics "wildness," "savagery," and "timidity." Yerkes (4) found a well-marked inheritance of these three traits, defining them in terms of the behavior of his rats in a given situation. But although he claimed a high degree of internal correlation (measuring the same animals on successive days), his criteria show no advantage over casual observation. Thus "savageness" is indicated by "(1) biting; (2) exposing or gnashing the teeth; (3) jumping at hand or forceps; (4) squeaking." Characteristics

given for wildness and timidity are of an analogous sort. But the lists are not mutually exclusive and involve some fine distinctions, such as between attempts "to avoid the experimenter" (timidity) and "to hide from view" or "to escape from the hands or the forceps" (wildness). M. P. Sadownikowa-Koltzowa (2), in describing an experiment similar to Miss Vicari's, concludes that any analysis of the maze behavior of her rats must be made in terms of a multiplicity of factors. She suggests the implication of three sets of genes: "1. die Aktivitätsgene, 2. die Gene der Furcht und Wildheit, 3. die Gene des Anscheininstinktes." She offers no evidence for the unity of these characteristics. Their existence is derived here, as with Yerkes, from inspection.

Nevertheless wildness, savagery, and timidity are not empty terms, and it is conceivable that they will ultimately submit to analysis and definition. Moreover, from our present knowledge of the hereditary transmission of the gross behavior which they describe, it is certain that some of their component parts show genetic constancy. The experimental demonstration of this must wait for methods of isolation and measurement. Meanwhile any attempt to show the inheritance of such a thing as ability to learn or intelligence must meet the necessity of excluding these grosser factors. With the maze this seems an insuperable difficulty.

The correlation within each group which Miss Vicari obtained (not calculated, but apparent in her curves) can be attributed to so many incidental factors showing genetic differences that we scarcely need discuss the matter further. It would appear that the reaction-times of a wild and of a tame mouse (roughly speaking) show a difference which depends upon the reactions of each animal to incidental stimuli (such as the release, the closing of doors to prevent retracing, and the like). A further difference in the records would arise from the fact that in a new situation a tame mouse runs about freely, while every movement of a wild mouse is either checked by strong inhibitions or frustrated through lack of direction. Such an influence would weaken with time and may account for the slopes of two of the present "curves." In addition to these matters of "temperament," there are others which must have affected such a measure as Miss Vicari's "reaction time." The degree of stereotropism [Crozier and Pincus (1)] shown by each strain is important, since the peculiar construction of the maze presented an array of surfaces and open spaces. Again, differences in receptors alone would account for such internal correlations against strain differences as were found. The Japanese waltzing mice, already handicapped by a difficulty in equilibration, are also deaf, and thus less responsive to auditory cues or disturbances. The abnormal-eyed are either blind or nearly so.

It is conceivable that learning might be so defined as to embrace some of these factors, but it cannot include them all without serious distortion. Miss Vicari's work, nevertheless, is offered as evidence of the inheritance

of degrees of learning ability, and even of the dominance of one type of learning curve over another. It is difficult to find the warrant for these conclusions in the data presented. However careful the handling of genetic material may have been, the experiment seems irreparably damaged through the lack of an accurate and unequivocal method of measurement.

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EXPERIMENTAL EVIDENCE IN SUPPORT OF PROFESSOR THURSTONE'S CRITICISM OF THE PHI-GAMMA HYPOTHESIS

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Some time ago Professor Thurstone (1) called attention to the fact that "the phi-gamma hypothesis is at best an approximation formula and that it is theoretically incorrect." He shows that if the limen is an increasing function of the stimulus then a psychometric curve must be positively skewed. Since the phi-gamma curve is symmetrical, it cannot possibly fit an actually obtained discrimininal series. The discrepancy between the phi-gamma curve and the psychometric curve is rather difficult to detect in the case of fine discriminations but will be very apparent in the case of coarse discriminations.

It is the purpose of this paper to show that certain experimental data demonstrate with clearness the validity of Professor Thurstone's criticism. Evidence will be presented from two sources, (a) an experiment in discrimination of circles using white rats as subjects, and (b) an investigation of short time-intervals reported by Woodrow (2). In both investigations the discrimininal functions were found to be crude.

DISCRIMINATION OF CIRCLES BY WHITE RATS

Two mature female rats were given 50 trials per day on the problem of choosing the larger of two circles. Circles (variables) of the size indicated in Table 1 were cut in heavy cardboard. Each variable was exposed